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CONTENTS.

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Walker Prizes in Natural History.

The **Boston Society of Natural History** offers a first prize of from $60 to $100 and a second prize of a sum not exceeding $50 for the best memoirs, in English, on one of the following subjects:

1. The relations of inflorescence to cross-fertilization illustrated by the plants of Eastern Massachusetts.

2. What depths of formerly overlying rocks, now removed by denudation, may be inferred from the structure of various rocks in Eastern Massachusetts?

3. Experiments affording evidence for or against the theory of evolution.

Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed by a motto corresponding to one borne by the manuscript, and must be handed to the Secretary on or before April 1, 1894.

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SAMUEL HENSHAW, Secretary.

Boston, July 3, 1893.

NEW METHOD OF PROTECTING BUILDINGS FROM LIGHTNING.

SPARE THE ROD AND SPOIL THE HOUSE!

Lightning Destroys. Shall it be Your House or a Pound of Copper?

PROTECTION FROM LIGHTNING.

What is the Problem?

IN seeking a means of protection from lightning-discharges, we have in view two objects,—the one the prevention of damage to buildings, and the other the prevention of injury to life. In order to destroy a building in whole or in part, it is necessary that work should be done; that is, as physicists express it, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider in this place; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings. The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life.

Why Have the Old Rods Failed?

When lightning-rods were first proposed, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy—heat, electricity, mechanical power, etc.—were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that the electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,—"to draw the lightning," as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, "Can an improved form be given to the rod, so that it shall aid in this dissipation?"

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod, suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed—will be readily dissipated—when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a bell wire) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread on a board. The objects against which the conductor rests may be stained, but they are not shattered.

I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor,—a conductor so strong as to resist the explosive effect,—damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the London Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. . . . No part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall."

One hundred feet of the Hodges Patent Lightning Dispeller (made under patents of N. D. C. Hodges, Editor of *Science*) will be mailed, postpaid, to any address, on receipt of five dollars ($5).

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AMERICAN LIGHTNING PROTECTION CO.,
874 Broadway, New York City.

SCIENCE

NEW YORK, JULY 14, 1893.

THE WRENS OF TRAVIS COUNTY, TEXAS.

BY CHARLES D. OLDRIGHT, AUSTIN, TEXAS.

1. *Catherpes Mexicanus conspersus*, Cañon Wren. This bird is an "endemic" species, its occurrence in any district depending on the topographic features. The great rock walls of the Colorado River, and the numerous side cañons form an ideal dwelling-place for this little bird, and here it may be found at all seasons, and its loud, ringing song re-echoes from cliff to cliff in the dreary days of November as well as in April's sunshine. But it penetrates into the city, and every morning this year one of the first sounds that I have heard has been the matutinal song of a cañon wren whose nest was in a cranny of an unoccupied house standing next to mine.

The cañon wren (as active a busy-body as the rest of his tribe) seems to be never too tired to sing. Reclining on the soft grass at the margin of the rivulet you look up the great frowning cliff and see a tiny bird, now clinging to the perpendicular rock, now disappearing in some crevice of the cliff and then perching on a projecting fragment, he utters a succession of clear bell-like notes in a descending scale

As this wren usually nests in some crevice far up in the cañon wall its eggs are often safe from the hands of the oölogist. Many times have I gazed longingly at a few straws projecting from a hole, while the owner of the nest watched me complacently. In such cases "'tis distance lends enchantment to the view." However, I have had the pleasure of examining several nests containing eggs and young, and as I have never seen any detailed account of the nidefication of this species, I will give some particulars about them.

This bird begins building early in the season, a nest with hatching eggs in it having been taken on the 30th day of March. In 1890 fresh eggs were found April 3, 4 and 11.

The nest is placed in some cranny or hole of convenient size, always in the face of the cliff; other situations are on a rafter in a barn, under the cornice on a veranda and in the chimney of an uninhabited house.

The nest is composed of grass and weeds and lined warmly with hair, wool and cotton. The complement of eggs varies from three to five, four being perhaps more usual than either of the other numbers.

The eggs always have a clear white ground, while the markings vary from a very slight sprinkling of brown pin-points to numerous large blotches and spots of reddish-brown and lilac, forming a confluent ring encircling the crown; this is the most common pattern of coloration. Their shape is ovate or rounded ovate, but I have seen one pyriform egg in the nest with three other normally shaped eggs.

2. *Phryathorus ludovicianus*, Carolina Wren. An abundant bird in the bottom lands along brooks and in all heavily timbered country. The Carolina wren is another fine singer, but spends too much of its time in scolding owls, crows and men. But often, especially in the spring and at sunset on a summer's day, one of these birds will perch on the topmost twig of a tall shrub and will, with his tail drooping and head thrown back, call "sweet William" until the woods resound. By the way, "sweet William" does not express the exact sound of the bird's notes to me, but I am so hopeless of expressing birds' voices by English words that I will not attempt to amend it.

This bird cannot be called particular in its choice of a nesting-place, for their nests have been found in hollow logs, under the cornice of a house, in a tin can placed in a tree, in a hole in a rock wall and on the window sill of a farmhouse. The hollow log is, I believe, the most usual situation. The nest is made to fit the cranny in which it is built and generally fills it. Twigs, grass weeds, leaves, hair, cotton, wool, rags, paper and even other materials enter into its composition. In shape it is more or less rounded, with an entrance in the side. The eggs are four, five or six in number, five being most common.

There is not much variation in the eggs; the markings being in some heavier than in others. The ground color is white, spotted thickly and finely with specks of reddish-salmon color and lilac, generally forming a poorly defined ring around the crown. The ground color is usually well concealed.

Fresh eggs may be found from April 1 to May 15, the height of the breeding season being during the middle of April.

3. *Thryothorus bewickii murinus*, Baird's Wren.

Probably our commonest wren, found in all kinds of country, bottoms or uplands, forest or prairies, mountains or plains. I believe, however, that Baird's wren prefers a broken country, little patches of prairie and mesquite groves alternating with the timber.

A number of these birds must spend their whole lives in the city of Austin, for in nearly every garden one may find a pair.

They are fussy little creatures hardly ever silent for a moment but keeping up a lively chatter or querulous "chee, chee, chee." But all through the spring, even as early as January, the males are great singers, and early on an April morning one cannot go far without hearing the sweet and cheerful song of one of these little birds. At such times one finds the bird perched in a tree-top, but on other occasions he will be hopping amongst the bushes or along a rail fence. flirting his long tail, uttering a continuous "chirp, chirp," and at each third "chirp" stopping a moment to pour forth his little ditty. This is kept up for hours at a time.

In February the wrens become restless and may be seen promenading the back yards in pairs peering into every hole and bird-box. They seem to be often undecided as to a nesting place, for I have known of a pair starting four different nests within a week, without any apparent cause for their fickleness. Any place seems good enough for this bird to start a nest—though as I have just stated they are more particular about its final location. Many people here have put small wooden boxes at their gates for the reception of mail matter, and I verily believe that each one is looked into once a year by a Baird's wren, with a view to building in it, and indeed many are chosen as nesting sites.

The nest is simply a mass of rubbish—but always warmly and softly lined with feathers or cotton. Six is a common complement of eggs, but as many as eight or as few as four may constitute a full set. The eggs are white, more or less speckled with brown of varying shades and lilac, sometimes the specks of reddish brown are thickly and uniformly distributed, again they are collected into a ring surrounding the crown or else rather larger specks of chocolate brown and lilac shell markings are more sparingly disposed.

Two "albino" eggs came under my notice last spring; one was immaculate white, the other had a very faint speckling on the crown; both these eggs were with other normally colored eggs. I also found a peculiar "runt" egg of this species, it is of normal coloration but measures only .55 by .44, being thus the size of a humming bird's egg. I found it one day in a hole in a telephone pole, and left it thinking that more eggs might be laid, as I saw the birds at hand; but when, after the lapse of several days, none were deposited, I took it. Why the bird laid no more I do not know. Surprise at the first one may have had something to do with it.

4. *Troglodytes aëdon aztecus*, Western House Wren.

Of this member of the family I can say but little, for during his winter stay with us he is very silent and indeed shy.

I am aware that he, like his kinsmen, can scold with remarkable vehemence, for I have heard him. While he remains with us he is to be found in the creek bottoms wherever there are

thickets of brush-wood. He remains with us until late in the spring, indeed the other wrens have young ones before he thinks of leaving for his northern "summering place." Last year I saw some on the 22nd of April. I sent one of them to Washington where the "bird doctors" pronounced it "aztecus."

5. *Salpinctes obsoletus*, Rock Wren.

This bird hardly deserves a place to itself, being quite uncommon and differing little in appearance and mode of life from the Cañon wren, which seems to represent it with us. It is more common further west. Indeed, this is the most easterly record in Texas of its occurrence.

METALLIC CARBIDES.

BY F. P. VENABLE, CHAPLE HILL, N. C.

THIS name is given to compounds formed by the direct union of carbon with the metals. They are not numerous nor do they seem to be easy of formation and it is very difficult to prepare them in a pure and definite form. Consequently they have been but little studied so far. None of them are known to occur in minerals of terrestrial origin.

Interest in these bodies has been heightened of late by the discovery of new ones, and by the instructive decompositions of some of them.

First as to the general mode of formation. They are usually formed by the action of intense heat upon the metal in the presence of carbon. The form of this carbon is capable of being greatly varied. Graphite, amorphous carbon and many hydrocarbons can be used. The carbide is especially formed when the metal is being extracted from its compounds, that is, in the nascent state. Several metals thus unite with carbon in the process of manufacture, as zinc, copper and notably iron, and the presence of the carbides renders the metal hard and brittle. The purification and analysis of these bodies is not at all an easy problem, and hence little or nothing is known of their formulas or chemical constitution. Five or more formulas have been assigned to iron carbide, and, of course, several may exist, still the correctness of any of these formulas is questionable.

The heat of the ordinary furnace is sufficient to form the carbides of the metals already mentioned. For others, more recently discovered, as the carbides of aluminium, of calcium, of barium, etc., the intense heat of the electric furnace is necessary. The first of these, aluminium carbide, is a most interesting body, of a light golden yellow color, it can be gotten from the electric furnace in a mass of corundum and metallic aluminium. It was described first by Sterry Hunt. Though it will stand intense heat in the air without appreciable change, yet really it is undergoing change all the time as is proved by the odor of hydrocarbons coming from it and the fact that left to itself in air it crumbles in a few weeks into a mass of white alumina. A few shining golden scales of the pure substance can be separated, but so far no analysis has been given to the world.

All of these carbides, under certain conditions, give off their carbon in the form of hydrocarbons. The same smell can be detected in all during their decomposition. In some cases, as iron and zinc, the decomposition is caused by the action of an acid. The carbides of the earths decompose in moist air and more rapidly in water. Calcium carbide decomposes the most energetically of them all. The evolution of the hydrocarbons would be called violent. Of course, the hydrogen needed for the reaction comes from the decomposition of the water or from the hydrogen acid.

A most interesting fact recently published in the scientific journals, is that the calcium carbide on decomposition yields lime and pure acetylen gas. The acetylen seems very pure. A thousand cu. cm. of the evolved gas was passed into an ammoniacal solution of copper chloride, and not a bubble went through. All was absorbed and precipitated. This is very important because the modes of preparing acetylen in common use are tedious or expensive, and hence this important hydrocarbon has not been as carefully studied as it otherwise might have been.

The formation of hydrocarbons by the decomposition of iron carbide has furnished a basis for one of the theories as to the origin of petroleum. If great quantities of iron carbide existed beneath the earth's surface and were subjected to decomposing influences, such oils and gases as are found in petroleum regions might very easily be formed.

So far there has been little utilization of these carbides commercially. One of the purer forms of iron carbide is used in a process for preparing metallic sodium, and the iron carbide in cast iron confers upon it many of its useful properties. If these bodies can be produced cheaply enough, however, there is strong probability that certain of them will prove very useful.

PHILOSOPHY IN THE COLLEGE CURRICULUM.

BY HOLMES DYSINGER, CARTHAGE COLLEGE, CARTHAGE, ILL.

STUDIES under the name of philosophy are to be found in almost every college curriculum. Either because the subject is too vague or abstruse for the comprehension of the average student, little more than elementary psychology, which is rightly regarded as a necessary part to the introduction to the subject proper, and a brief discussion of practical ethics, are taught in most of the schools outside of the few real universities. While the number of subjects advocated for introduction into the college course is increasing constantly, one so fundamental as philosophy should not be neglected. Apart from its theoretical value, it has practical bearings upon the intellectual range of a man, regardless of the system he adopts, that commend it to the thoughtful consideration of educators.

The subject-matter with which philosophy deals bears a peculiar relation to all other subjects in the course, in as much as its office is, partly at least, to systematize and explain all the principles of the particular sciences. This gives the unity so desirable in a course of study, and so essential to the thoroughly-trained mind. From this it serves the highest purpose in education and deserves a prominent place in every course of liberal culture.

The philosophical powers of man are last in order of development. The subject-matter makes it necessarily so. It is the most abstruse of all forms of knowledge. The mind in its unfolding passes up through perception and conception to the realm of widest generalizations and the discovery of the principles that are assumed in all our thinking. Philosophy deals with forms of knowledge that stand at the farthest remove from that furnished in so-called presentation — the first development in the mind's unfolding.

When the mind reaches that stage of development in which it apprehends the principles fundamental to all knowledge, it turns in upon itself and critically examines its own processes and assumptions to determine the certainty of being and the validity of our knowledge. This is the highest stage in man's intellectual ascent. Here he stops. He has completed the circuit of the globe of knowledge. He started with the facts furnished in sense and consciousness, and ends in the principles that underlie and embrace all knowledge. These stand accredited in his own thinking. Beyond this the mind of man cannot penetrate.

That many students cannot attain this stage of knowledge is evident to all who have taught the upper classes in our colleges; that but few who attempt it get further than the outer court, is to be expected; but that all are greatly benefitted intellectually would not be denied by those who have looked into the merits of the case and examined the evidence with impartiality. A few additional facts will give our reasons for this conclusion.

Notwithstanding its abstruseness, as a discipline in thinking and in logical method, philosophy has no equal. Facts as furnished by the senses and distinguished from principles are not dealt with in philosophy, but the relation of facts to one another and to all things else. All these in a system of philosophy deserving of study or worth elaboration must be included in their relations of coördination and subordination. The unity of all being is the ultimate problem of philosophy. A narrower range and lower ideal may satisfy science, but it cannot attain to that which comprehends all knowledge. Only the mind well disciplined in logical method can grasp the facts, but the one who attempts to do so will develop a power that is the possession of few and the desire of all.

This apprehension of facts as related is essential and necessarily precedent to the discovery of principles which govern these relations. In this respect practical fruit is to result from the study of philosophy. Not simply philosophers, but even the students of philosophy, must get a more comprehensive grasp of facts and principles, as each is assigned its place in the whole system of knowledge. Truth is apprehended in its harmonies and wholeness. It is seen in its proportions.

If more attention were given to a careful study of philosophy as a system, rather than in its history, much of the conceit of knowledge which is so prevalent to-day would be unheard of. The specialist would soon discover that he was occupying a very small niche in the universe of knowledge; the broadest scholar that his horizon included but an infinitesimal portion of the sphere of truth.

BRITISH STONE CIRCLES. — III. DERBYSHIRE CIRCLES.[1]

BY A. L. LEWIS, TREASURER ANTHROPOLOGICAL INSTITUTE, LONDON, ENGLAND.

THE Peak district of Derbyshire, so justly famed for its scenery, possesses also many attractions for the archæologist, among which are two stone circles.

The larger of these, called Arbor Lowe or Arbe Lowe, is about six miles from Bakewell, and consists of an oval ring, the diameters of which were about 126 and 115 feet, the precise lengths being difficult to ascertain in consequence of the stones, which doubtless originally stood upright, being now all flat, and having fallen, some outside, some inside, and some across their original positions, while others are broken into fragments or buried in the soil. There were perhaps about forty stones, of which nearly thirty remain entire or in fragments, the largest being about twelve feet long, six broad, and four thick. The longest diameter of the oval ran nearly northwest and southeast, and somewhat more to the west and east, two of the stones seem to have stood back outside the regular line of the oval. Within the oval, and on the line of the longest diameter, but not in the centre of it (the distances from the northwest and southeast ends being in about the proportion of three to two), are the remains of some large stones — one fourteen feet long — which were apparently three in number, forming a "cove," ⊏, like that in the centre of the northern circle at Abury, the central stone of which faced the rising sun on Midsummer Day. Like the circles at Abury, the stones at Arbelowe are surrounded by a ditch, which is about seven feet deep and fifteen wide at the bottom, outside of which is an embankment, formerly perhaps ten feet high and eight wide at the top; Sir G. Wilkinson says somewhat more, but it may be that he took the maximum and I took the minimum of the measure. This embankment is now very irregular, and in one place a tumulus has been formed from the materials composing it, in which were found two Celtic vases and a bronze pin. This tumulus could hardly have formed part of the original plan of the monument, and would therefore seem to have been made after the latter had fallen into disuse. The embankment, like that at Abury, is not a true circle, and there is much similarity in the irregularities of both, but that may be quite accidental. There are two entrances, one southeasterly, in the same direction as the Kennet entrance at Abury, and one to the northwest, but not quite opposite to the other; altogether Abury and Arbelowe, notwithstanding the great difference between them in size, have more points in common than any other circle has with either. Just outside the southeast entrance are two small stones, quite as likely to have been taken from the interior as to be in their original places. Nearly three hundred yards to the southwest is a tumulus, called Gib Hill, about twenty feet high and as wide at the top, in which a small cist was found, two feet under the surface, which contained a vase, two worked flints, and an iron fibula with places for stones — probably a secondary interment. A bank of earth of doubtful antiquity runs from the embankment for some distance in a direction south of Gib Hill. These various earthworks have been supposed to give the form of a serpent to the monument, but Sir Gardner Wilkinson's plan shows this idea to be quite incorrect; this is a point for the visitor to verify.

On the moors at the top of the hills above Eyam is a small circle of a different character from Arbelowe; it is called the "Wet Withins," and consists of a bank of earth, about six feet wide and two high, inside which, but close to the bank, was formerly a ring of small stones about two feet high and of proportionate size, of which ten remain, out of perhaps twenty or more. The diameter of this circle is about one hundred feet, and some sixty feet to the north-northeast there is a barrow, eighty-three feet long (from northeast to southwest) and forty-six feet wide.

There are some other small remains of a similar character in Derbyshire, but I have not seen them myself, and doubt whether they are worth the trouble of a visit.

[1] No. 1, Abury, appeared in No. 529, March 24; No. 2, Stonehenge, appeared in No. 537, May 19. To those who may wish for more minute details of measurements than can be given in a short article, I would recommend "Stonehenge," by Professor Flinders Petrie, D.C.L. (Stanford, London).

CHARAKA SAMHITA.

BY F. A. HASSLER, M.D., PH.D., SANTA ANA, CAL.

THE student of Hindu literature has before him an ever-widening field of research. He must be prepared for glimpses and magnificent views of learning and wisdom which will astonish and delight him at every turn. The thoughts and the meth d of expression are different from those of other nations, and there is scarcely a subject, except, perhaps, electricity and steam, that has not been discussed by these ancient sages. The philosopher will find his theories, the anarchist his ideas, probed to the bottom, and thestudent of the supreme soul, high, noble thoughts, and even from this grand subject down to the every-day question of mistress and maid, we do not think of any matter that will not be found fully investigated in the pages of the Mahabharata.

So the physician of our day will find in the Charaka and other works of ancient India many views of health, disease, and remedies which he fondly imagined were jewels in the crown of modern science. When a young man wishes to study medicine, he may receive a little instruction from his pr ceptor, but places his chief reliance upon the teachings of some medical school from which he receives his diploma. This was not the custom in ancient India. There were no colleges. Every student became a part of his preceptor's household, was lodged and fed by him, and beyond a few light services was not asked for any return. It is plain that such teachers could not instruct all their scholars by word of mouth. This accounts for the immense number of medical works of ancient India.

We cannot tell the age of the Charaka, it is based upon a work of Agniveca, which carries us back to almost mythical times. The very name of this supposed author sounds like the mystery of long past ages, for it may be translated "the dwelling-place of fire." Ten years of study of the Mahabharata has led me to quite certain conclusions as to the time when that great work was written, and I should say that the style, of the first part at least, of the Charaka corresponds with that portion of the Mahabharata which I think was written about the sixth century before Christ, or, in other words, about the time of the rise of Buddhism. Whatever its age may be, this we know, it is exceedingly ancient. It is mentioned by Avicenna, Rhazes, and others, and is supposed to have been translated by the early Persian and Arabian writers on medicine. But we forget its age when we read its pages. The work is immense. An English translation, now being published by Doctor Kiviratna, the learned editor of several Sanscrit works and of a medical journal in Bengali, will probably cover from fourteen to fifteen hundred royal octavo pages. But it is not its size to which I wish to call attention, it is the wisdom and learning found in it that make it so valuable and interesting.

In a short article like this I cannot expect to do more than give the reader a glimpse of the work and a quotation here and there. We are told that in the earliest times some fifty-odd learned men assembled to study the science of life and the causes of disease; in fact, it was a medical convention similar to those of our day. The first conclusion they arrived at was that — "Freedom from disease is the excellent root of religion, profit, pleasure, and salvation. Diseases are depredators thereof, as also of happy life. This, therefore, is a great enemy of men that hath appeared.

What shall be the means of checking them? Having said this, they betook themselves to meditation."

They did not discuss questions of life and health only, but moral and religious subjects also, and their effect upon life in general. The wind, or breath, disorders of the billiary system and phlegm, or improper secretions, seem to have been fully recognized as causes of bodily diseases, while passion and darkness of mind brought about mental disorders. Long lists of drugs and directions for their proper use are given, and there is abundant evidence that the properties of vaccine matter were well known. We are told that "He who knows how to apply these in disorders is conversant with the science of medicine." And listen to the following in regard to drugs and those who use them: "He who is acquainted with their applications according to considerations of time and place, after having observed their effects on individual patients, should be known as the best of physicians. An unknown drug is like poison, or weapon, or fire, or thunder, while a known drug is like nectar. Drugs unknown by name, appearance, and properties, or misapplied even if known, produce mischief. Well applied, a virulent poison, even, may become an excellent medicine, while a medicine misapplied becomes a virulent poison. Only a physician who is possessed of memory, who is conversant with causes and applications of drugs, who has his passions under control, and who has quickness of decision, should, by the application of drugs, treat diseases."

Thirty-two kinds of powders and plasters and six hundred purgatives are next described, after which a chapter on food and its proper use gives us as good advice as is to be found in any treatise published in this learned nineteenth century. Great stress is laid upon the proper care of the teeth, and a list of plants is given from which brushes can be made, there not being manufactories of such articles as there are now.

"As the chief officer of a city protects his city, as the charioteer protects his chariot, after the same manner should the intelligent man be attentive to everything that should be done for the benefit of his own body." Therefore, bodily, mental, and, if we may so call it, religious hygiene is discussed at length, and many excellent rules given.

The question of the duality of the mind and of its connection with the understanding and the soul leads us into all the intricate mazes of Hindu philosophy but are here discussed in such a lucid manner that one is not bewildered and can easily follow the line of thought with pleasure and profit.

"The objects of the mind are ideas. Here, again, the proper, excessive, scant, and injudicious correlation of the mind with its objects, or of the mental understanding with its objects, becomes the cause of the normal or abnormal condition of oneself." In other words, a man is sane or insane according to the proper or improper agreement of the mind and its ideas, the ideas the understanding conceives; and, therefore, "One should act in such a way as to preserve one's normal condition, in order that one's untroubled senses and mind might continue in an untroubled state; that is to say, by keeping oneself in touch with such objects of the senses as are productive of beneficial results; by properly achieving such acts as deserve to be achieved (and abstaining from such acts as should be abstained from), repeatedly ascertaining everything by a judicious employment of the understanding; and, lastly, by resorting to practices that are opposed to the virtues of the place of habitation, season of time, and one's own particular nature or disposition (as dependant upon a preponderance of this or that attribute or ingredient). Hence all persons desirous of achieving their own good should always adopt with heedfulness the practices of the good."

Selfishness was never a cause of happiness, and we are told "one can never be happy by taking or enjoying anything alone without dividing it with others." And this advice is good in every age of the world — "one should not trust everybody, nor should one mistrust everybody."

Hindu works teach that everyone should have complete mastery of his body and his senses, hence we frequently come across such a sentence as this: "One should not suffer oneself to be overcome by one's senses."

A very interesting chapter is that which treats of "The Aggregate of Four," that is, "the physician, nurse, drugs, and patient." Each is considered and as good advice as can be found given for the guidance of three of the aggregate. One thing, the first of the four, is taught which it were well to remember in our day; that is, that time must be considered in the treatment of all diseases, and one must not try to force a cure.

It would take more time and space than are at our disposal for us to consider all of even the four parts of the Charaka that have been published so far, but if any of our readers are interested, we would be glad to give them any information in regard to the work or the other publications of the learned editor of this great monument of ancient Hindu wisdom and leaning.

A NEW THEORY OF LIGHT SENSATION[1]

BY CHRISTINE LADD FRANKLIN, JOHNS HOPKINS UNIVERSITY, BALTIMORE, MD.

THE reasons which make it impossible for most people to accept either the Hering or the Young-Helmholtz theories of light sensation are familiar to every one. The following are the most important of them:

The Young-Helmholtz theory requires us to believe: (*a*) something which is strongly contradicted by consciousness, viz., that the *sensation* white is nothing but an even mixture of red-green-blue *sensations;* (*b*) something which has a strong antecedent improbability against it, viz., that under certain definite circumstances (e. g., for very excentric parts of the retina and for the totally color-blind) all three color-sensations are produced in exactly their original integrity, but yet that they are never produced in any other than that *even* mixture which gives us the sensation of white; (*c*) something which is quantitatively quite impossible, viz., that after-images, which are frequently very brilliant, are due to nothing but what is left over in the self-light of the retina after part of it has been exhausted by fatigue, although we have otherwise every reason to think that the *whole* of the self-light is excessively faint.

The theory of Hering avoids all of these difficulties of the Young-Helmholtz theory, but at the cost of introducing others which are equally disagreeable; it sins against the first principles of the physiologist by requiring us to think that the process of building up highly organized animal tissue is useful in giving us knowledge of the external world instead of supposing that it takes place (as in every other instance known to us) simply for the sake of its future useful tearing down; it necessarily brings with it a quite hopeless confusion between our ideas of the *brightness* and the *relative whiteness* of a given sensation (as is proved by the fact that it enables Hering to rediscover, under the name of the specific brightness of the different colors, a phenomenon which has long been perfectly well known as the Purkinje phenomenon); the theory is contradicted (at least the present conception of it) by the following fact—the white made out of red and green is *not the same thing* as the white made out of blue and yellow; for if (being mixed on the color-wheel) these two whites are made equally bright at an ordinary intensity, they will be found to be of very different brightness when the illumination is made very faint.

Nevertheless, the theory of Hering would have to be accepted if it were the only possible way of escape from the difficulties of the Young-Helmholtz theory. But these difficulties may be met by a theory which has the following for its principal assumptions.

In its earliest stage of development vision consisted of nothing but a sensation of grey (if we use the word grey to cover the whole series black-grey-white). This sensation of grey was brought about by the action upon the nerve-ends of a certain chemical substance set free in the retina under the influence of light. In the course of development of the visual sense the molecule to be chemically decomposed became so differentiated as to be capable of losing only a part of its exciting substance at once; three chemical constituents of the exciter of the grey-sensation can therefore now be present separately (under the influence

[1] Abstract from the Proceedings of the International Congress of Experimental Psychology, London, 1892.

of three different parts of the spectrum respectively), and they severally cause the sensations of red, green and blue. But when all three of these substances are present at once they recombine to produce the exciter of the grey sensation, and thus it happens that the objective mixing of three colors, in proper proportions, gives a sensation of no *color* at all, but only grey.

This theory is found, upon working it out in detail, to avoid the difficulties of the theories of Helmholtz and of Hering.

Its assumption of a separate chemical process for the production of the sensation of grey gives it the same great advantage over the Young-Helmholtz theory that is possessed by the theory of Hering; it enables it, namely, to account for the remarkable fact that the sensation of grey exists unaccompanied by any sensation whatever of color under the five following sets of circumstances—when the portion of the retina affected is very small, when it is very far from the fovea, when the illumination is very faint, when it is very intense, and when the retina is that of a person who is totally color-blind. This advantage my theory attains by the perfectly natural and simple assumption of a *partial* decomposition of chemical molecules; that of Hering requires us to suppose that sensations so closely related as that of red and green are the accompaniments of chemical processes so dissimilar as the building up and the tearing down of photo-chemical substances, and farther that two complementary colors call forth photo chemical processes which destroy each other, instead of combining to produce the process which underlies the sensation of grey.

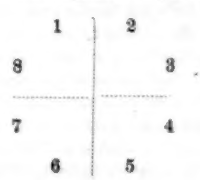
Of the first four of the above enumerated cases the explanation will readily suggest itself; in the case of the totally color-blind it is simply that that differentiation of the primitive molecules by which they have become capable of losing only a part of their exciting substance at one time has not taken place; the condition, in other words, is a condition of atavism. In partial color-blindness and in the intermediate zones of the retina in normal vision the only colors perceived are yellow and blue. This would indicate that the substance which in its primitive condition excites the sensation of grey becomes in the first place differentiated into two substances, the exciters of yellow and blue respectively, and that at a later stage of development the exciter of the sensation of yellow becomes again separated into two substances which produce respectively the sensations of red and of green. In this way the unitary (non-mixed) character of the sensation yellow is accounted for by a three-color theory as completely as by a four-color theory. A three-color theory is rendered a necessity by the fact that it alone is reconcilable with the results of König's experiments for the determination of the color-equations of color-blind and of normal eyes,[1] experiments which far exceed in accuracy any which have yet been made in color-vision, but which, owing to the intricate character of the theoretical deductions made from them, have not hitherto been allowed their due weight in the estimation of color theories.

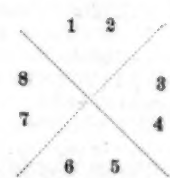
The explanation which the theory of Hering gives of after-images and of simultaneous contrast are not explanations at all, but merely translations of the facts into the language of his theory. My theory is able to deal with them more satisfactorily; when red light, say, has been acting upon the retina for some time, many of the photo-chemical molecules have lost that one of their constituents which is the exciter of the red sensation; but in this mutilated condition they are exceedingly unstable, and their other two constituents (the exciters of the sensations of blue and of green) are gradually set free; the effect of this is that, while the eyes are still open, a blue-green sensation is added to the red sensation with the result of making it gradually fade out into white, and, if the eyes are closed, the cause of the blue-green sensation persists until all the molecules affected are totally decomposed. Thus the actual course of the sensation produced by looking at a red object,—its gradual fading out, in case of careful fixation, and the appearance of the complementary color if the illumination is diminished or if the eyes are closed,—is exactly what the original assumption of a partial decomposition of molecules would require us to predict. The well-known extreme rapidity of the circulation in the retina would make it impossible that the partly decomposed molecules just referred to should remain within the boundaries of the portion of the retina in which they are first produced; and their completed decomposition after they have passed beyond these boundaries is the cause of the complementary color-sensation which we call simultaneous contrast. The spreading of the actual color which succeeds it would then be accounted for, as Helmholtz suggests, by a diffusion of the colored light in the various media of the eye.

No effort has hitherto been made to explain a very remarkable feature in the structure of the retina,—the fact that the retinal elements are of two different kinds, which we distinguish as rods and cones. But this structure becomes quite what one might expect, if we suppose that the rods contain the undeveloped molecules which give us the sensation of grey only, while the cones contain the color molecules, which cause sensations of grey and of color both. The distribution of the rods and cones corresponds exactly with the distribution of sensitiveness to just perceptible light and color excitations as determined by the very careful experiments of Eugen Fick.[2]

Two other theories of light sensation have been proposed besides the one which I have here outlined, either one of which meets the requirements of a possible theory far better than that of Hering or of Helmholtz; they are those of Göller[3] and Donders.[4] The former is a physical theory. That of Donders is a chemical theory, and very similar to the one which I here propose. Every chemical theory supposes a tearing down of highly complex molecules; Donders's theory supposes, in addition, that the tearing down in question can take place in two successive stages. But Donders's theory is necessarily a four-color theory; and Donders himself, although the experiments of König above referred to had not at that time been made, was so strongly convinced of the necessity of a three-color theory for the explanation of some of the facts of color-vision that he supplemented his four-process theory in the retina with a three-process theory in the higher centres. The desirableness, therefore, of devising a partial decomposition of molecules *of such a nature* that the fundamental color processes assumed can be three in number instead of four is apparent.

But the theory of Donders is open to a still graver objection. The molecules assumed by him must, in order to be capable of four different semi-dissociations, consist of at least eight different atoms or groups of atoms. The red green dissociations and the yellow-blue dissociations we may then represent symbolically by these two diagrams respectively:





But it will be observed that the two completed dissociations end by having set free *different* combinations; in the one case 1 is combined with 2 and in the other case 1 is combined with 8, etc. If, now, the partial dissociations are so unlike as to cause sensations of yellow and blue (or of red and green) it is not probable that completed dissociations which end in setting free *different* chemical combinations should produce the *same* sensation, grey. The difficulty introduced by Donders's theory is therefore (as in the case of Hering's theory) as great as the difficulty sought to be removed. It is the desire to secure the advantages of a partial dissociation theory, without the disadvantages of the theory of Donders, that has led me to devise a partial dissociation of molecules of a different kind. The theory will be found more explicitly set forth in the next number of the *Zeitschrift für Psychologie.*

[1] A König und C. Dieterici. *Sitzungberichte der Berl. Akad.* vom 29 Juli, 1886.

[2] Studien uber Licht und Farbenempfindung. *Pflüger's Archiv*, Bd. XLIV., s. 441, 1888.

[3] Die Analyse der Lichtwellen durch das Auge. *Du Bois-Reymond's Archiv*, 1889.

[4] Noch einmal die Farben-systeme. *Gräfe's Archiv für Ophthalmologie*, Bd. 30 (1), 1884.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

THE CAPABILITIES OF PHOTOGRAPHY NOT UNLIMITED FOR ILLUSTRATING ALL CLASSES OF OBJECTS.

BY O. G. MASON, OFFICIAL PHOTOGRAPHER AT BELLEVUE HOSPITAL, NEW YORK CITY.

THE comparatively recent departure from old methods in various fields of scientific research, has called into action agencies for solving problems of initial progress and results not known or utilized by earlier workers. Discoveries within the last few years have so advanced the lines of study, and an active scientific press has scattered so broadcast the knowledge of progress made that, although the field is boundless, he who reads has little excuse for reworking ground from which all reachable fruit has been gathered. In eagerness for the new, a desire to find some hidden, shorter paths into the mysteries of nature, do we not often fail to recognize obstacles, or to sufficiently consider the best means for their removal? With pen and pencil our predecessors sought to leave a record of their work. What they thought and what they saw have been handed down to us through the best means at their command. For the physician, the botanist, mineralogist, and the geographer the artist sketched, elaborated, and finished illustrations having a more or less amount of truth, often obscured by some personality, which rendered them valueless or even misleading. In no class of objects have such defects been more conspicuous than that requiring the use of the microscope. Therefore, he who had used with dissatisfaction the hands of the draftsman was eager to utilize the means offered by photography. He had seen the results obtained in other fields, and, without knowing the difficulties in the way, believed it easy to obtain all desired brilliancy, detail, and amplification. It may be asked, Why have not these expectations been more fully realized? When we pause to consider that color is a most important feature in photographic work, and that a majority of objects studied under the microscope reflect or transmit the least actinic rays of light, red, orange, green, and yellow, we may well understand why we do not secure brilliancy. Again, when the microscopist studies his subject for detail, he mentally eliminates all those parts which do not belong to the special point under observation. A crystal, cell, or fibre which over- or underlies his object or forms a full or partial background in the field of the objective is left out in the mental summing up of his study. The laws of chemistry and optics do not permit such selection and elimination from the photographic mage. A slight tremor conveyed to the microscope by a passing vehicle in the street, a step about the room or house, may be annoying to the observer, but does not prevent securing results by longer application. But when we consider the necessity of absolute immobility of the instrument, often for a considerrble length of time, in order to impress upon even the most sensitive plate the image of many-colored objects, we can well understand one of the greatest causes of failure to secure detail; and this obstacle of motion becomes far greater as the amplification increases. It is plain that motion is multiplied equally with the diameter of the object; or, in other words, if we magnify an object one thousand diameters, a motion of that object to the extent of one-thousandth of an inch becomes in the amplified image a motion of one inch, which very readily shows why good results cannot be obtained under such conditions. When observing with the microscope, it is possible and quite feasible to focus the instrument above and below the general plane of the object, in order to study any projecting points which may be within or without the general plane. This feature is not possible with the photographic process, save in so far as diaphragming the lens and modifying the light may effect the result. *Over*estimation of the possibilities of photography and *under*estimation of the careful preparation of objects have occasioned much unnecessary labor and great disappointment by failure to produce results which should be sought through different channels. When the investigator contemplates the employment of photography for illustrating his work, let him consult his photographer before preparing his objects. No one human being has yet encompassed *all* that is known. When the anatomist takes to his photographer a *thick* section of muscular or ossified tissue and asks to have the individual striæ and cells isolated and delineated with distinct outlines and minute detail, he will fail to realize his expectation. When the mineralogist or geologist prepares his sections of crystallization or deposits, he must not calculate that *all* his various planes will be perfectly shown in one photograph, even if the specimen *be* translucent. Color, mass, and position are important factors in all photographic work. With orthochromatic plates many objects heretofore impossible of proper illustration may be quite successfully treated; but, with objects of this class, another factor, that of time of exposure, offers a barrier of limitation. The mobility of life, animal and vegetable, is a most important element which cannot be ignored in exposures of hours, or even minutes, and seconds. A vegetable fibre, when placed in concentrated light, may make one or more entire revolutions during the time of exposure necessary to properly impress its image upon an orthochromatic plate; and especially is this the case when a high-power objective is used. Thin sections devoid of the less actinic colors, red, orange, yellow, and green in their darker tints, or admixtures, may be easily treated. Circulating fluids or objects changing size or position are susceptible of instantaneous exposures only. When such objectionable features as motion and non-actinic color are present, the problem becomes far more complicated, and if the photographer fails in its clear and complete solution his patron sometimes looks upon such failure as a proof of incompetency or a lack of proper effort. Like her sister handmaids in the advance and illustration of scientific thought, photography stands ready to do her proper work. She has done much, and it is believed will do more to enlarge the field of human knowledge and gather the harvest; but we should not ask her to accomplish the impossible.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Worms on the Brain of a Bird.

In the issue of *Science* for June 2, is a short account of my finding thread worms in the brain cavity of *Boturus mugitans*. The title of the article should have read "on" instead of "in," as they were not in the tissue of the brain but, as I state there, in the subarachnoid space.

Since writing the short article above referred to I have received a card from Professor J. W. P. Jenks of Providence, R. I., in which he gives an account of his investigation of a similar if not the same parasite on the brain of the Snake Bird (*Plotus anbingus*). To quote a little from his communication, he says:

"In 1874 I camped for 50 days near Lake Akechobee in south Florida, and shot dozens of the Snake Birds, and in 19 out of 20 mature birds found a bunch of 10 to 20 parasitic worms just beneath the arachnoid membrane, but in no instance extending

into the substance of the brain. In young just hatched I never found any. In young from two to three weeks old I found them in their stomachs and the alimentary canal. When about ready to fly I found coiled perhaps two or three on the brain."

Further on in his note to me he says: "I was surprised to learn of your finding them in *Boturus*—but I should not have been for I consider them primarily a fish parasite and developed from the eggs taken with the fish into the stomach of the bird, and hence like *Trichina spirulis* finding their way to the brain."

Professor Jenks called my attention to a note he published on this find in his "Popular Zoology," but which I had overlooked. He also gave me the address of Dr. W. Cahall of Philadelphia who had published an article on the subject, based largely on the material Professor Jenks obtained from Florida. There is only one point in Dr. Cahall's article (Journal of Nervous and Mental Diseases for June, 1889), that I wish to speak of, and that is that while 19 out of 20 Snake Birds have these brain parasites they do not seem to affect them unfavorably. This was not the case with the Bittern. It was poor in flesh, of inferior size and deficient in intelligence.

That birds do get parasites from fish I might add the following case of circumstancial evidence: When skinning a perch (*Perca flavescens*), I found in the muscles a number of encysted parasites, the cysts white and about an eighth of an inch long. A short time afterwards in skinning a wild duck I found a similar if not the same parasite in the pectoral muscles. The two parasites were of the same size and color and seemed to be the same.

G. H. FRENCH.

Carbondale, Ill.

The International Botanical Congress at Madison.

IN looking over the "Circular and General Programme of the Forty-Second Meeting of the American Association for the Advancement of Science" just distributed, I am surprised to read on page 12, under the heading "International Botanical Congress," the following statement: "The congress will consider questions of general botanical interest, but papers embodying the results of research will be excluded. The International Standing Committee upon Nomenclature, appointed last year at the Genoa Congress, is expected to present a report at this time." This is all that is said in the circular to indicate what we may expect to hear at the Congress.

The *Botanical Gazette*, in an editorial,[1] urges "If any botanist has a suggestion . . . now is the time to give it expression. . . . Silence means apathy." I fear a certain class of our botanists have been silent too long, judging from the above statement. It seems to me outrageous to announce a programme from which all original research is excluded. No scientific man cares to listen to papers which are merely "a play of words," not the results of research. I should consider it an insult to our foreign guests to offer such a programme. The one subject suggested, *nomenclature*, is indeed about the only one possible under such restrictions, being truly void of all scientific research.

Botanical congresses do not come every year, especially in America, this being the first ever held here, if I am rightly informed. This being the case, it seems to me, as a matter of course, that this should be the time and place for a discussion of the vital questions of physiology, morphology, anatomy, etc., that this should be the time for an extreme effort on the part of every American botanist. If we desire to gain standing as true botanists among the true botanists abroad, our supreme effort should be directed to *botany*, not as appears to be the intention, to a mere machine of botany. It would seem a better restriction if all papers *not the result of research* were excluded.

Papers from America have long presented this characteristic — no "result of research." Nomenclature and floristic is truly all that we have thus far accomplished. One is, unfortunately, compelled to believe that "Free Lance"[2] accidentally omitted to include botany when he said: "The Entomological Society is recruited very largely from the ranks of 'collectors' who notoriously infest entomology far more than any other branch of natural history." The omission is at least unfortunate. The following sentences of the paragraph are so pithy and to the point that I cannot refrain from quoting them also: "The great majority of these have probably no interest in science generally, but care only for those things relevant to butterfly collections (herbaria, in our case). They would never become Fellows of the Linnæan, and care chiefly to discuss 'collectors' topics, that would be quite out of place in that society; so that the Entomological Society affords them a sort purgatorial limbo, midway between the paradise of science and the inferno of popular nescience."

I trust that I misunderstand the word *research* as used by the committee, but it would seem desirable that they should better explain what is meant. It may be intended that all papers containing research should be presented to Section G of the American Association, fearing that if the congress were not restricted Section G would be scantily patronized. This, however, does not seem a reasonable interpretation, for if there is a limitation on the congress, we should expect it to be open only to the best papers of most general interest, which could readily be decided by a committee on programme; lesser papers and papers of local interest being referred to Section G.

The claim cannot be made with justice that nomenclature has more than a factional interest. The majority of good botanists of the world pay no attention to nomenclature, and to them a discussion of its intricacies would be dry and worthless in the extreme. If such factional questions are to be the only ones considered, the congress should not be called a "Botanical Congress," but a *Nomenclature Congress*. Whatever may be intended, it is an unfortunate use of words.

It is announced that a separate circular will shortly be distributed to botanists, giving further information. It is to be hoped that a clear explanation of this point will be given.

H. J. WEBBER.

Subtropical Laboratory, U. S. Department of Agriculture, Eustis, Fla.

[1] Botanical Gazette, vol. xvii. (November, 1892), p. 384.

[2] "On the Organization of Science," by A. Free Lance, Edinburgh, 1892, p. 25.

A Plea for a Fair Valuation of Experimental Physiology in Biological Courses.

DURING the discussion of the biology question, one point has interested me more than any other, namely, that none of the parties who have taken part in the discussion have been able to avoid speaking at the same time of evolution and of natural selection. This thinking of biology, with constant reference to those two features of Darwinian teaching, has led me to believe more strongly than ever that my view of the matter is not very much wrong. However, an article in this journal, entitled "Biology in our Colleges: A Plea for a Broader and More Liberal Biology," induces me to take up my pen once more and explain matters a little more closely.

The tendency of the above-named paper "is — a plea for systematic biology," but it is marked by such a number of wonderful views on the different lines of physiological investigation that many specialists will really be at a loss about what they shall think. "Systematic zoölogy has gone, or, if still tolerated in a few colleges, is restricted to a very subordinate position." I imagine that the biologist would not know what to do if systematic work, both zoölogical and botanical — the latter holds still, says the article, "an honored place in many universities, though evidently on the wane" — was not carried on, so that we could know how to lay our hands upon the different forms for further study. But the methods of such a work may be wrong, and, fatally, often are so, namely, when it presents itself merely as simple regristation work, which strikingly has been called museum zoölogy or botany. Systematic work of any kind is to be valued just as much as morphological or physiological work, and so, even if it is done still — as in fact it is in ninety-nine cases out of a hundred — after the old Linnæan principles. On the other hand, a biological classification, or even only a morphological classification, which employs biological characters of the forms, is to be more highly valued.

There is no doubt but that any naturalist enjoys the "delight

n contemplating the aspects of nature," and "derives enjoyment from studying the forms, habits, and relationships of animals and plants," but how can he do so, and thus become a "biologist" unless he peers "through the tube of a compound microscope," etc., and does his proper hardening, and staining, and "monographs the same bit of tissue." How such investigations can "obscure the objects" we are trying to explain is rather a mystery. If, at least, anybody allows them to obscure our general views, there can be no speaking of scientific work. Natural history has become, in our century, so broad that no man possibly can become a "general naturalist" or a good "faunal naturalist" any more; he will, at least, not be able to treat all the questions that arise in any other way but in that of the amateur. The objects of our investigations lie a little deeper than to glance at all that is "most beautiful" and attractive to the eye.

How the article comes to the conclusion that the study of the minute structure is histology or that of development embryology, is rather doubtful. Further, I am anxious to know if any of the readers walking over the scientific border-land commanded by the naturalist who might be educated according to the principles given in the article of which we speak did ever meet with "the various pathogenic micrococci of fermentation and disease" which are mentioned (p. 353). However, I shall not enter upon further details, but turn towards the view expressed in the said article about "section-cutters and physiologists," and I shall try to show that the work done by the workers in this particular field is far from being one-sided, at least, when we are speaking of real scientific men who put an equally fair valuation on all of the branches of their science. There are, as Professor E L. Greene said, "a good many men trying to figure somewhere" as scientific writers, but where are the scientific men to be found when we look towards the "scientific border-land" (Greene)? Therefore, we shall see that the right sort of scientific physiologists do not dare to depreciate any of the branches of their science.

Professor P. L. Panum once said that he who would not acknowledge physiology as the fundament of pathology and of the other departments of medical science has no right to be called a scientist. The vegetable physiologist who does not know anything about the principles of agriculture, horticulture, and forestry also loses this right, and so he does, if he is ignorant with regard to a great deal of the practical, industrial branches. If we go to the opposite side, he must know how to carry out more minute investigations; he cannot avoid being something of a "slice-cutter," and if he should be unfortunate enough to find "some new form of cell or new property of protoplasm," he must understand how to trace such a discovery as far as it can be traced. I am, therefore, very much surprised to hear that "the modern school of histologists, under the head of biology, teach little besides the minute structure and function of tissues." For my personal account, I have studied physiology almost from the time when I could appreciate the blessings of the study of natural history, but I have never met a man who claimed to be a physiologist,— *in casu* vegetable physiologist,— and who, speaking, for example, of the nitrogen question, did not know the theoretical investigations quite as well as the practical experiments with fertilizers. But it must be noted that natural science has, at present, reached such an extent that no man possibly can cover the whole ground. Thus we have, with regard to special work, to become specialists, and, therefore, it is possible to take a farmer's boy and make out of him "a general naturalist of the present day" or a "local faunal" — or floral — "naturalist." He will be no scientific man.

"Biological" teaching is a failure for other reasons than those presented in the article. A college professor may offer a course in "general biology" and include "cell structure and the structure of the less complex tissues of animals and plants." But this is not "general biology;" the structure of two different forms has not the least to do with biology, it comes under the heading of internal or external morphology, and, when making a study of this kind, the student does not see more of life in general and of the laws by which it is governed than he saw before. Here the experimental physiology of animals and plants must be held up before a school of "biologists" who are following a phantom of their own imagination if they really believe that function can be explained out of form. It is here that "the pendulum has swung too far," and it is not in the direction of "exclusive microscopic and physiologic work." The latter is safe enough. The fault lies entirely in the methods of modern biology, which begins with giving itself a wrong definition. If the modern biologist had cared more for experimental physiology, he would know now how to direct his actions when the pendulum "swings back."

If I understand the article aright, the student should begin his biological work with elementary "general biology." He will, then, come to the university without, practically speaking, knowing anything about "biological" questions, and he will plunge into the study of cell-structure at once. This beginning of a course would be anything but beneficial to the young, ignorant student. If we take the example of the farmer's boy, he would naturally have to start with the study of what we call external morphology, collect plants, insects, or shells, and perhaps study their ways. It would be entirely lost on him to train him in the study of the cell and its organs. The other special sides of biology which are proposed for study are: 2. Morphology, taxonomy, and relationships; 3. systematic work in widely-separated groups; 4. faunal work; 5. the distribution of life in time and space; 6. the principles and philosophy of biology.

These are the constituents of "biology!"

If it were so, the condition of natural science would be very lamentable. Not a single word or hint is given about the existence of experimental work, which should be the main factor in the whole course of training. It is true, as has been said, that "sham" is a hard expression, but here it might be used very properly. Many of the "biologists" of the present day will hardly understood my view, because they have been taught to regard the study of morphology as the essential part of their biological studies, but the physiologists will do so, because they know that we can take but very few steps in any direction without experiment. So long as biological courses do not include a proper course in experimental physiology of animals and plants, they cannot be called properly scientific. Anybody who will not believe this may be referred to Paul Bert's "La Science Experimentale."

There is no danger that I should have misunderstood the article. I see clearly that it wishes the "systematic biology," which might have been called, more logically, biological classification, to take a place a little more ahead of what it holds at present. But, trying to give a fair valuation of the other branches of physiology, it fails entirely. It is well known how language can command the thoughts, and if biologists go forth without knowing what they are teaching, the present confusion will grow instead of being settled. Perhaps "biology" will gain more and more lovers and become (as it is) very fashionable, but the amount of restless work, chasing new problems and pursuing all that is interesting merely because it is new, will not, in time, be very much valued. Nothing can save "biology" except experimental physiology.

J. CHRISTIAN BAY.

Missouri Botanical Garden, July 7.

Mr. McGee and the Washington Symposium.

It strikes me as curious, and certainly contrary to scientific usage, that the succinct statements made by Mr. King as to the limitations of his inferences on the earth's age are ignored by our Washington friends. One might actually imagine that we were not on the scent of polymerism [1] considered either with reference to its volume or the inseparable thermal effect; or that we were unaware of the high pressure and long range thermal variations of the physical constants of rocks. It takes so little time, so little cerebration to adduce critical commonplaces of this nature,

[1] If there was one subject in which we imagined that our work had reached the point of prolixity, it was the change of chemical or molecular constitution as resulting from temperature and stress. (Cf. Am. Journ., xxxiii., p. 28, 1887; ibid., xxxvii., pp. 339, 351, 1889; ibid., xlii., p. 498, 1891; ibid., xliv., p. 242, 1892; etc.; Phil. Mag., xxxi., p. 9, et. seq., particularly § 25, 1891; ibid., xxxv., p. 174, § 3, 1893; Am. Chem. Journal, xii, p. 1, 1890; Bull. U. S. Geolog. Survey, No. 94, 1892; and elsewhere). And now comes Mr. McGee with obviously well-meant instruction on the feasibility of our polymeric mechanism.

that they are always bountifully forthcoming. But the things which one really wants, the physical character of an alleged discrepancy, its numerical value, the so-many per cent of error under such conditions,— these one is left to wish for in vain, supposing that one has not long since learned to pay the personal groaning for the personal satisfaction. So far as I am concerned, if I could not adequately state how big a sin it is under which somebody else is staggering, I should prefer to hold my peace, believing that matters of vague conjecture are not fit to be chronicled. Nobody on the same side of common sense would to-day attempt to exhaust so complex a problem as the one in question in a single instance. It is reasonable, however, to try to remove piece by piece, element by element. What we did was an endeavor to remove the preponderating element, and I must reiterate that if our respite had not been cut short by recent unfavorable legislation, other things would have been brought out in their turn and in due time. Perhaps it is heresy to state that an immense future awaits laboratory research in physical geology; but stating it, one would like to refer not so much to the punching of clay or the pulling of taffy candy, as to legitimate physical measurement However, others have survived even the odium of cultivating "exact" methods. We are soothing ourselves with the comfort of so thinking.

CARL BARUS.

Phys. Laboratory, U. S. Weather Bureau, Washington, D. C.

The Lac de Marbre Trout, A New Species.

DESCRIPTION: B. 11 12; D. 13; A. 13; V. 9; P. 14; Vertebræ, 60.

The specimen described is about twelve inches in length. Body subfusiform, compressed, pointed at snout, slender at the tail. Height of body near one-sixth of the total length; head one-fifth, crown convex. Snout one and one third, and interorbital space one and one-half times the eye. Eye little less than one-fifth of the head, two-thirds of the space between the orbits on the forehead. Mouth large; maxillary straight, extending backward almost as far as the hinder edge of the eye, bearing strong teeth on its lower edge for nearly its entire length. Teeth on intermaxillary and mandibles stronger. The tongue bears a series of four strong hooked teeth at each side, and behind the glossohyal on the basibranchials there is a band of several series of smaller ones. Gill rakers straight, short, sharp, rough, 8 + 14 on the first arch. Opercle thin, with a few striæ. Scales very small; apparently there are about two hundred and thirty in the series immediately above the lateral line and more than two hundred and fifty in a row five or six scales above this. Distance from first ray of dorsal to end of snout little more than that from the same ray to the tip of the adipose fin. The middle of the total length falls halfway between the ends of the hinder rays of the dorsal and its base. Dorsal and anal fins are slightly emarginate at the ends of their median rays. Pectorals and ventrals small; base of latter slightly behind the middle of that of the dorsal. Caudal pedicel slender, notch very deep, hinder border sinuous, as in Salmo alpinus, lobes pointed. The caudal notch is deeper in this species than in any other of the American forms except S. namaycush.

Back dark brown with an iridescent blueish tint, unspotted. Dorsal dark, clouded, without spots or bands. Pectorals, anal and ventrals orange in the middle, yellowish or whitish toward bases and at their margins. The dark color of the back shades into whitish tinged with pink below the lateral line. Ventral surface white, no doubt reddish in breeding season. Head black on top, silvery on the cheeks, white beneath. Flesh pink. Caudal fin yellowish toward the base, brown toward the hinder border, which has a narrow edging of light color. Faint areas of lighter tint suggest a few spots of red in life along the lateral line; the condition of the specimens is such that this may be left in question, as also the number of caeca or presence of parr-bands of which there are faint indications.

This fish is evidently allied to the blue-back of the Rangeley Lakes, S. oquassa, but reaches a greater size than that species, and is readily distinguished by the maxillary and its dentition, the caudal fin, and the coloration. Similarly when compared with S. arcturus, S. stagnalis and S. Rossi, it is seen to be quite distinct. With the saibling, S. alpinus, introduced in Sunapee Lake and elsewhere, it has still less in common.

Our specimens were taken in Lac de Marbre, Ottawa County, Province of Quebec, Canada, whence they were sent by favor of the Hon. J. G. A. Creighton. They reached us at the instance of Mr. A. N. Cheney, fishing editor of *Shooting and Fishing*, who when asked to suggest a specific name replied with the question, "How would it do to name it for Mr. R. B. Marston, editor of *Fishing Gazette*, London, an Englishman overflowing with good feeling for everything pertaining to fish, fishing and America, and who is doing much to enhance friendly interest between the people of the two countries?" In consequence of the suggestion this handsome char, one of the handsomest of our species, is introduced under the name, *Salmo (Salvelinus) Marstoni*.

S. GARMAN.

Mus. Comp. Zool., Cambridge, Mass.

Tucumcari.

THE writer first visited this historic locality in 1887, before he had had opportunity to define the Denison beds at the top of his Lower Cretaceous section in northern Texas, and fell into the error, which others have not escaped, of concluding, from the peculiar Jurassic-like *Gryphæa dilatata*, Marcou, the only fossils found upon that visit, that the beds were Jurassic, and so published his opinion.

Later, however, after having had an opportunity to complete his study and arrangement of the stratigraphy of the Comanche series in central Texas, he discovered in the Denison beds[1] of his Washita Division certain features which led him to believe that his early diagnosis of the Tucumcari beds was erroneous, and that they were really closely allied in age to the Denison beds. Under this impression, which was communicated orally to all interested, he availed himself of the first opportunity to revisit Tucumcari, April 30, 1891. He then discovered in association with *G. dilatata* the list of additional species herewith given, and, at earliest opportunity, under date of May, 1892, published, in a general discussion of the region, the following revision of his previous conclusions, which was the first printed announcement of the Cretaceous age of the *G. dilatata* beds: —[2]

"The Trinity Sands and Red Bed Regions.

"The writer has twice visited the Mesa Tucumcari and found it a most interesting geological remnant of the former area of the Llano Estacado. The table or summit described by Capt. Simpson is covered with the typical Llano Estacado formation, identical in composition and formerly continuous with the sheet which covers the Llano proper, some 20 miles distant. Below this is a vertical escarpment of 50 feet or more of typical Dakota sandstone resting upon loose sands and clays, forming a slope identical in aspect and fossil remains with the Denison beds of the Washita Division, which have been eroded away from the 400 miles intervening between it and the main body of those beds at Denison, Texas. Beneath this is a large deposit of the typical Trinity sands *country*[3] of white pack sands, thin clay seams and flagstones, while the base is composed of the typical vermilion sandy clays of the Red Beds."

Notwithstanding the above clear statement of my opinions, the Third Annual Report of the Geological Survey, printed nearly a half-year afterward, devotes many pages to asserting that I held to the Jurassic age of the *G. dilatata* beds at Tucumcari. Upon pointing out this misquotation, instead of acknowledging the error, and repairing the injustice, it was followed up by a privately

[1] Denison beds as originally defined and used by writer. Not the Denison beds of Taff, as used in an entirely different meaning. Compare Bulletin of Geological Society of America, Vol. II., p. 501, and Third Annual Report of Texas State Geological Survey.

[2] "On the Occurrence of Artesian and Other Underground Waters in Texas, Eastern New Mexico, and Indian Territory West of the 97th Meridian," by Robert Thomas Hill (being part of Vol. III. of Senate Document 41, 1st Session, 52d Congress, Washington, May, 1892.

[3] For "country of" read "consisting of"— a typographic error.

printed, bitter, and vindictive attack upon my report, endeavoring to discredit all the work I had done in the Texas region. This last-mentioned paper is so utterly incorrect in its assertions, and so malicious in tone, that I do not think it needs other answer than a perusal of it. Certainly it has no place in scientific literature, and if any of my friends should be so deceived by it as to believe any of its assertions, I shall be glad to clear any doubts by correspondence.

In *Science* of May 26, 1893, p. 283, the author of the foregoing attacks again misquotes me by saying that after my second visit to Tucumcari I again affirmed Marcou's reference, an assertion which has no foundation, for hardly had the two lines after my first visit been printed before I realized my mistake, and orally communicated it to everyone interested, and have never since maintained by word or pen, and was the first to publish the true age of these beds.

It was impossible, in a general report written upon the subject of Artesian Water, to go into controversy over the age of a fossiliferous horizon. I had given a full outline of the region with its broader problems in a Bulletin of the Geological Society of America for 1891, entitled "Notes on the Texas New Mexico Region." In this paper I clearly set forth the Tertiary age of the Llano Estacado, and amplified many points which have since been published entirely *de novo*. Inasmuch as several parties have criticised me in public print for not giving the minutiæ of Tucumcari, I submit the following amplification of my previous remarks, and hope it will prove satisfactory to all fair-minded readers.

Section of Tucumcari Mesa.

Preliminary.	Thickness (estimated on spot).
6. Summit of Mesa (Neocene). White, calcareous, silicious, marly limestone of character peculiar to Tertiary formations of Great Plains	25–30
5. Escarpment around summit of Mesa (Dakota). Consisting of the massive brown-yellow sandstone, which I had traced for days from LaMora, and other points on the Las Vegas Plateau, and which Stevenson had called (I think properly) Dakota. Estimated to be about	75
4. Crumbling yellow sandstone at base of above, and (4a). Gentler slope, forming bench around summit escarpment, (Washita) Division of Comanche series. Decomposing sandstone of base of 4, and arenaceous clays and marls. Containing fauna of Denison beds, Washita Division at top, and *G. dilatata*, Marcou, in debris, apparently weathered out	100
3. Shoulder at base of above. Impure, yellow, arenaceous stone	15
Pedestal, or lower slope of Mesa.	
2a. Upper part (Trinity). White and red unconsolidated sands (pack sands), with thin strata of dimension-layers of hard quartzitic rock, and thin layers of blue clay, resembling in general character the Potomac sands of Maryland and the Trinity Sands of Texas. This horizon contains a peculiar granular mineral, resembling red coral, and outcrops in all the escarpment of the Las Vegas Plateau on the north side of the Canadian, and is denominated the white band in that region, to distinguish it from the brown band (Dakota) and underlying Red Beds	150
1(b) Lower portion of slope (Pre-Cretaceous). Bright, vermilion, argillaceous clays of the Red Beds continuing to bed of Canadian	250

The above section is not final or complete in details of the individual beds, but it illustrates the sequence of the four great formations as preserved at Tucumcari and in the adjacent Llano Estacado, and shows the geologic position of the following fauna, which were collected near the summit below the base of the sandstone escarpment which surrounds it, in beds numbered 4 and 4a.

List of Fossils.

1. *Turbinolia texana*, Conrad. United States and Mexican Boundary Survey.
2. *Ostrea (Gryphæa) dilatata*, Marcou. Geology of North America.
3. *Ostrea quadricostata*, Shumard. Transactions Academy of Science, St. Louis, 1860.
4. *Plicatula*, species undescribed.
5. *Neithea occidentalis*, Conrad. United States and Mexican Boundary Survey.
6. *Trigonia emoryi*, Conrad. United States and Mexican Boundary Survey.
7. *Protocardia multistriata*, Con. United States and Mexican Boundary Survey.
8. *Turritella marnochii*, White, or *Seriatim granulata*, Roemer.
9. *Ammonites leonensis*, Conrad. United States and Mexican Boundary Survey.

In addition to the above there are four species of Pelecyopoda, which I am unable to determine generically, but they resemble *Astarte*, *Lucina*, *Panopæa*, and *Isocardia*.

All of the species enumerated, with the exception of No. 2 (*G. dilatata*, Marcou), occur elsewhere in the greatest abundance and similarly associated in the Washita Division of the Comanche Series of Texas and Mexico, and, with the exception of Nos. 5 and 8, have never been found in any other beds than those of the Washita Division. Nos. 5 and 8 range downward into the Fredericksburg Division.

No. 1 (*Turbinolia texana* Con.) has not been reported east of the Pecos, but it occurs near El Paso, and at Arivichi, Sonora (as shown by Gabb), associated with a fauna similar to that of Tucumcari.

The forms from No 2 to No. 9, inclusive, are the most common and characteristic species of the Washita Division, and can be collected at nearly any locality where the entire division is exposed, between Marietta, Indian Territory, and the Rio Grande.

The ammonite is the common, characteristic ammonite of the Fort Worth beds of the Washita Division, at Denison, Fort Worth, Austin, and elsewhere, and has hitherto not been found except in the Fort Worth beds of the Washita Division.

Ostrea quadricostata, Shum., *Trigonia emoryii*, Con., and the other species mentioned are especially characteristic of the Denison or uppermost beds of the Washita Division, at Denison, and hence my reference of these beds at Tucumcari to the Denison beds of the Washita Division.

As I have previously maintained, *G. dilatata*, Marcou, is a good species, entirely distinct from *G pitcheri*, Morton, and, as has been said, has remarkable resemblance to the Jurassic *G. dilatata* of Sowerby. Under these conditions it is not strange, then, that before the stratigraphic and paleontologic position of the Washita Division was known, that the distant Tucumcari beds should have been adjudged Jurassic upon the evidence of the two species collected therefrom by Marcou, which certainly have, when considered alone, a most Jurassic aspect.[1]

The section and list of fossils above given differ in detail from those published on page 208 of the Third Annual Report of the Texas State Geological Survey. The two lists, however, both show the *Gryphæa dilatata* beds to be of the age of the Washita Division of the Comanche Series, and the author of the Texas report, which was printed several months after the writer's, came to the same conclusion, although he seems to have been unaware of the fact that the writer had abandoned his early reference of the *G. dilatata* beds to the Jurassic. With the exception that the beds which the writer refers to the Trinity, are referred by the Texas author to the Triassic, there is no dissimilarity between their conclusions.

Following is the list of fossils published in the Texas reports, "collected from the Tucumcari beds in the vicinity of Tucumcari

[1] *Gryphæa dilatata*, var. *tucumcari*, Marcou, and *O. marshii*, Marcou.

and Pyramid Mountains." It is unfortunate that the exact locality of the collection is not given:—

Gryphæa dilatata var. *tucumcari*, Marcou.
Ostrea marshii, as determined by Marcou.
Gryphæa pitcheri, Morton,
Exogyra texana, Roemer.
Ostrea quadriplicata, Shumard.
Trigonia emoryii, Conrad.
Cardium hillanum, Sow.
Cytherea leonensis, Conrad.
Turritella seriatim granulata, Roemer.
Pinna, Sp.
Ammonites.
Pecten.

Finally, the writer wishes to state that he is not prepared, nor does he desire, to write a final treatise on the Tucumcari, which can never be properly related until the atlas-sheets of the United States Geological Survey are completed for the region. Tucumcari is but a single station in the vast group of phenomena belonging to the deposition and degradation of the Las Vegas and Llano Estacado Plateaus and the Canadian Valley, and to be properly understood, it would be necessary to write a treatise on the whole region. One thing is settled beyond all doubt in my mind, however, and that is that the *G. dilatata* beds of the region do not belong to the Jurassic, but are undoubtedly of Cretaceous age. On the other hand, it may also be safely assumed that the *Gryphæa dilatata*, Sow., of Marcou, is not the same as *G. pitcheri*, Morton, as has been asserted by many authors, nor does it occur in the Cretaceous beds of central Texas, so far as the writer is aware. But this is a question which cannot be discussed intelligibly until a thorough revision of the Gryphæas is made.

In conclusion, permit me to say that there is not one trace of the Jurassic formation over the Texas region, as Mr. Marcou so positively affirms, and, furthermore, that there is no evidence that it was ever there, the whole trend of the testimony being to show that that region was land during the Jurassic period.

If the writer should devote his time to criticising the works of his contemporaries or predecessors, he would have little time for research. It has been my practice, however, under the opinion that all knowledge is progressive, to see the good in the works of others, and to correct any errors without abuse. In all I have published on the Texas region, there is not a line which was written with the desire to discredit any man, and yet I believe that my severest critics will confess that there has been great advance in opinion since I undertook the renaissance of geologic study in Texas.

My collections from Tucumcari are in Washington, and are open to the inspection of anyone interested. ROBT. T. HILL.

Chloropia.

THE case of Wallian, reported on page 360 of the latest volume of *Science*, would seem to be one of temporary *Chloropia*. More extended and carefully recorded observations, while the observer is looking at various objects under various conditions, would be very desirable. E. W. SCRIPTURE.

Yale University, New Haven.

Trees as a Factor in Climate.

I ONCE observed a signal case of the effect of trees in determining rainfall. A few years ago I was walking along a road in the so-called backbone of England at an elevation of from 800 to 1,000 feet above the sea-level. It was a dull, calm October day, and the hills on either side were cased in mist. Where I was no rain was falling and the ground was quite dry. As I passed on the road entered and traversed a wood of fir trees. Here I at once encountered a gentle drizzle. Far from suspecting that the trees were playing any part in the matter, I concluded that the expected wet weather had at last set in. When the road emerged from the wood at its opposite extremity I found that no rain was there falling or had fallen. Still I did not connect the trees with the downfall, but imagined that the weather had again improved.

On returning from my destination about three hours afterwards I found that the rain was still falling in the wood, but that it ceased as soon as I emerged into the open country. The ground, too, within the wood was wet, still all around it was dry. Hence it appeared that a slight rain must have been falling for the greater part of the day within the wood, but not in the bare fields and heath land outside.

Thus under certain conditions of the weather the presence of trees may determine rainfall which would not take place in their absence. J. W. SLATER.

London, England.

Mineral Wax.

I notice an account and inquiry in *Science* of June 16 in regard to the receipt at the National Museum of specimens of natural wax coming from Portland, Oregon, derived from the shores of the Columbia River, and from other accounts it is found along the coast from the Columbia River to Puget's Sound.

The material has been well known for the past half century as mineral wax, native paraffin, ozokerite and lastly as ozocerite, a hydro-carbon compound (hydrogen, 15 per cent; carbon, 85 per cent — variable); supposed to be derived from bituminous and lignite coal formation by infiltration and crystallization. It is generally found *in situ* in the neighborhood of coal and lignite beds and in the bituminous clays or shales.

The legend as to its being derived from a wreck is a most absurd one. It is a resinous wax in consistency and translucency, with structure sometimes foliated; color brown or yellowish-brown by transmitted light; leek green by reflected light; odor, aromatic, in specimens that I have examined, having the characteristics and feel of beeswax that had been lying for some time in water.

It is mined in variable quantities in Germany, Austria, Turkey, and England, associated with the soft coal and lignite beds.

In Galicia alone about 30,000 tons have been mined since its discovery there in 1859. It is used in Europe principally in the manufacture of candles and by refining in place of beeswax and paraffin. It is said to be an excellent electrical insulator.

In the United States it is mined *in situ* at Soldiers Summit, Uintah County, and in Emery County, Utah. Sixty-five thousand pounds were marketed in 1888, with a yearly increasing output. The whole product of the United States in 1890, including the Oregon find, reached 350,000 pounds.

The imports of mineral wax, ozocerite, under the names of bay or myrtle, Brazilian and Chinese wax, in 1890 were over one and a half million pounds.

It has been found *in situ* in thin seams in the lignite beds of Oregon, Washington, and British Columbia. The deposits along the Columbia River and on the sea-shore of Oregon are no doubt the debris from lignite beds near by. C. D. HISCOX.

361 Broadway, New York.

BOOK-REVIEWS.

The Seismological Journal of Japan. Edited by JOHN MILNE, F.R.S.

IN 1880 the Seismological Society of Japan was founded by a number of earnest students of seismology in that country, prominent amongst whom was the editor of this Journal. In the earlier years of its existence its membership included such well-known names as Milne, Gray, Ewing, Mendenhall and others at that time resident in Japan, and their interest in the science led especially to the invention of many instrumental appliances for the study of earthquake phenomena, some of which have been copied wherever earthquakes are observed, and in some respects have revolutionized the science of experimental seismology. It also resulted in the establishment of a chair of seismology in the Imperial University of Japan, and the organization of a bureau controlling a central observatory and some 700 outside stations. Of late years, however, the interest in the society has declined, partly through the return of some of its most active supporters to England and America, and, after publishing sixteen volumes of Transactions, in 1892 the society ceased to exist. Professor Milne, however, still remains in Japan and has determined to continue the publication of seismological literature in the present

journal, which is therefore to be regarded, not as an entirely new venture, but as a continuation of the series heretofore known as the Transactions of the Seismological Society. The new journal is issued in the same form and from the same printers as the old Transactions, and the first number, now at hand, bears on its title page Vol. XVII, which is its number in the old series, so that the new volumes can be bound uniformly with those previously issued. The annual subscription is five dollars.

In this number the first article is on ' The Mitigation of Earthquake Effects and Certain Experiments in Earth Physics" by Professor Milne, in which various lines of experiment are proposed that might possibly lead to the prediction of severe earthquakes so as to guard against their effects. In the second, "On the Application of Photography to Seismology and Volcanic Phenomena," Professor W. K Burton describes with illustrations the photographic records from Milne's t emor indicators. In the third Professor Milne gives an abstract of the "Seismometrical Observations for the Year 1890," from which it appears that in that year 845 earthquakes were felt in Japan, of which 49 were classed as severe, 264 as moderate and 532 as feeble. Of the severe earthquakes, four (Jan. 7, Mar. 19, Apr. 16, Nov. 17) were accorded more detailed description. In the fourth article "On the Overturning and Fracturing of Brick and other Columns, by Horizontally Applied Motion," Professor Milne and F. Omori describe a very interesting series of experiments, wherein various objects such as blocks of wood of different dimensions, bricks, columns built of brick or of cement, were mounted on a wheeled truck to which a reciprocating horizontal motion could be communicated, and the circumstances of the motion, with the overturning or fracture of the object, were electrically recorded. From the data the maximum velocity and maximum acceleration necessary for overturning were calculated and compared with the experimental results with a fairly good agreement. In an article on "Earth Pulsations in Regard to Certain Natural Phenomena and Physical Investigations," Professor Milne concludes that "the movements called earth tremors are movements in the crust of the earth not altogether unlike the swell upon the ocean," and infers a connection between them and the steepness of the barometric gradient. In an article "On the Movements of Horizontal Pendulums," he gives an abstract with notes of certain observations made by Dr. E. von Rebeur-Paschwitz at Potsdam, Wilhelmshaven and Teneriffe, and published in the *Astronomische Nachrichten*. F. Omori gives "A Note on Old Chinese Earthquakes," and as the concluding article Professor Milne gives a twenty-page "Note on the Great Earthquake of October 28, 1891," the phenomena of which are further discussed in his report to the British Association, 1892, and the complete account of which is to be issued under the auspices of the Imperial University of Japan, but is not yet ready for publication. According to the statements of this account the killed numbered 9,960, wounded 19,994, and houses totally destroyed 128,750. The immediate cause of the disaster was the formation of a fault which can be traced on the surface of the earth for a distance of between forty and fifty miles, and shows a difference of level amounting in many places to twenty or thirty feet. There is also abundant evidence of horizontal displacements, sometimes as great as eighteen feet, and the whole Neo Valley appears to have suffered a permanent compression, becoming narrower, the piers of bridges being left closer together than before the earthquake. There were also many observations of surface waves in the earth, involving a perceptible tilting of objects resting upon it; and the maximum horizontal motion indicated by the instruments was from 25 mm to 35 mm; with a period of from 1 to 2.5 seconds.

Notions de Chimie Agricole. Par TH. SCHLOESING, FILS. Paris, Gauthier-Villars et fils. Quai des Grands-Augustins, 55. 208 p. 8°. Broché, 2 fr 50. Cartonné 3 fr.

FOLLOWING in the footsteps of the elder Schloesing, M. Th. Schloesing, Fils, Ingenieur des Manufacteurs de l'Etat, presents us with an admirable treatise on agricultural chemistry covering in detail the physical and chemical relations of soil and atmosphere

to plant life. Methods of chemical analysis are wisely left for a separate work and the results of analysis alone are given when a knowledge of the same is necessary to an understanding of the discussion. The nature of the experiments, however, and the manipulation of the same, are given with sufficient fullness to enable the reader to judge of the value of the conclusions. The general arrangement of the book is as follows: Part I. treats of the nutrition of plants, of germination, and of the origin of the organic and inorganic constituents. Part II. makes a study of the atmosphere in its relation to plant life and of the gases influencing this life, of nitrogen, oxygen, carbonic acid, nitric acid, ammonia, etc. Part III. treats of soils, their formation and composition, and of their physical and chemical properties. A bibliography, coinciding with the arrangement of the text, completes the work.

The author is particularly interesting in his section on nitrification and also in treating of the assimilation of free atmospheric nitrogen by plants and soils. The experiments and conclusions of Berthelot and André are noted as well as those of M. Schloesing, the author concluding with; "Il n'entre pas dans notre programme d'insister davantage sur ces diverses recherches; car nous tentons d'ordinaire à n'avancer que des faits positifs. Ici il ne nous est guère permis de faire un choix entre les opinions produites. Il est à espérer qu'un prochaine avenir levera les doutes qui règnent encore sur ce grave sujet."

The book has the usual exquisite neatness of first-class French publications, with full-bodied paper, clear print and broad margins, making it altogether a most enjoyable volume.

CHARLES PLATT.

Outlines of Forestry, or the Elementary Principles Underlying the Science of Forestry. By EDWIN J. HOUSTON. Philadelphia, J. B. Lippincott Co. 254 p. 12°. $1.

THIS little book is a useful manual of facts relating to the subject. Among the matters considered are the conditions necessary for the growth of plants, distribution over the earth, formation of soil, animate and inanimate enemies of the forest, vapor, rain, drainage, climate, hail, reforestation and tree planting, etc. The last chapter, called "Primer of primers," contains in short, concise sentences the substance of what had been given at length in the earlier chapters. Taken by itself, it would serve a useful purpose in the education of the general public to the importance of the subject.

The book is, perhaps, unfortunately written in a loose and rather slovenly manner. It abounds in repetitions of not only the same ideas, but also of nearly identical words. The following extracts are particularly bad examples, but they fairly represent the ordinary style of the writer: "Heat and light are to be found in practically all parts of the earth. They differ, however, in amount in different regions of the earth, and such differences cause the differences that are noticed in the plants that grow in different regions" "The quantity of moisture in the air differs greatly in different parts of the earth, and on this difference, together with the difference in temperature, depends the differences observed in the plants of various regions." "Each section of the country possesses, so to speak, a nationality in its plants, or, in other words, there lives in each section of country a particular nation of plants. Such a nation of plants, or the plants peculiar to a particular section of country, is called its flora."

The author makes use of a new word, "heatshine," which is rather difficult to define. "The sunshine and the heatshine which awaken the sleeping germ and call it into activity," etc. In the appendix are given various lists of trees suitable for planting, and these contain some curious errors. For example, under the head of "deciduous trees" we find maples, hickories, cedars firs and pines, while under "evergreens" are placed spruce, larch, sweet gum, poplar, oak, walnut, etc. In another place we observe under "conifers" bald cypress, red cedar, white pine, black cherry and European alder, while the European larch figures in another table as an evergreeen. Errors of this kind rather detract from the value of the book.

JOSEPH F. JAMES.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For sale or exchange.—A complete set of the report of the last Geological Survey of Wisconsin, T. C. Chamberlin, geologist. It consists of four large volumes, finely illustrated, and upwards of forty large maps and charts. Will sell for cash or exchange for a microscope. Address Geo. Beck, Platteville, Wis.

For sale or exchange for copper coins or rare postage stamps. Tryon's American Marine Conchology, containing hand colored figures of all the shells of the Atlantic coast of the United States. Presentation copy, autograph, etc. One vol., half morocco, 8vo, usual price, $2?, postpaid, $15. Botany of the Fortieth Parallel of the Hundredth Meridian of the Pacific R. R. Survey. Other Botanical works and works on Ethnology. F. A. Hassler, M.D., Santa Ana, Cal.

I have a fire-proof safe, weight 1,?50 pounds, which I will sell cheap or exchange for a gasoline engine or some other things that may happen to suit. The safe is nearly new, used a short time only. Make offers. A. Lágerstrom, Cannon Falls, Minn., Box 857.

For exchange.—Hudson River fossils in good condition from the vicinity of Moore's Hill, Ind., also land and fresh water shells. Desire fossils and shells from other groups and localities. Address Geo. C. Hubbard, Moore's Hill, Ind.

For sale at low price.—A fine old-fashioned photographic camera, rosewood box, one foot square, lenses, four inches diameter, made by C. C. Harrison. Plateholders, troughs, baths, etc., all in large wooden case, formerly the property of the late President Moore, of Columbia College. This is a fine example of an instrument of the best make for the old wet-process methods, and valuable to any institution or amateur interested in the history of photography in the U. S. Address M. S. Daniel, 286 W. 4th St., New York.

I wish to exchange a collection of 7,000 shells, 1001 species and varieties, American and foreign, land, fluviatile and marine, for a good microscope and accessories. Address, with particulars, Dr. Lorenzo G. Yates, Santa Barbara, California.

For exchange.—I wish to exchange Lepidoptera of South Dakota and other sections, for Lepidoptera of the world. Will purchase species of North America. Correspondence solicited, particularly with collectors in the Rocky Mountains, Pacific coast and Hudson's Bay regions. P. C. Truman, Volga, Brooking county, South Dakota.

Wants.

A YOUNG man who has been through the course in mathematics in Princeton University, wishes some tutoring this summer. Rates reasonable. Address P. H Westcott, Cramer's Hill, Camden Co., N. J.

A GRADUATE of an American Polytechnic institution and of a German university (Göttingen), seeks a position to teach chemistry in a college or similar institution. Five years' experience in teaching chemistry, Address Chemist, 757 Cary St, Brockton, Mass.

AN experienced teacher in general biology wishes a position in a first-class college or university. Three years in post-graduate study. Extensive experience. Strong indorsements. Address E. W. Doran, Ph.D., 1827 G St., N. W., Washington, D. C.

THREE teachers wanted for a male and female seminary in central New York. Typewriting, etc., languages, mathematics, sciences, *et. al.* Send stamp with and for particulars. Box 701, Hempstead, L. I.

A ZOOLOGICAL collector and taxidermist of ten years' experience in the field is now open to engagement, for either field or laboratory work. References furnished. Address Taxidermist, Box 75, White Sulphur Springs, West Va.

WANTED, as principal of a flourishing technical school, a gentleman of education and experience who will be capable of supervising both mechanical and common school instruction. Special familiarity with some technical branch desirable. Address, giving age, qualifications, etc., J. B. Bloomingdale, Fifty-ninth street and Third avenue, N. Y

WANTED.—A young man as assistant in our microscopical department. Queen & Co., Philadelphia.

THE undersigned desires specimens of North American Gallinae in the flesh for the study of their pterylosis. These species are especially desired: *Colinus ridgwayi, cyrtonyx montezumae, deudragapus franklini, lagopus welchi, tympanuchus cupido* and *pedioecetes phasianellus.* Any persons having alcoholic specimens which they are willing to loan or who can obtain specimens of any of the above are requested to communicate with Hubert Lyman Clark, 3922 Fifth Avenue, Pittsburgh, Pa.